be distinct realities, viz., Force and Energy. Further, he states that "the term 'potential' applied to Force or Energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it." This too is the view of Tyndall and Balfour Stewart. According to this doctrine, if I throw a stone upwards, say to the height of twenty feet, the energy expended is not lost, but gradually changed in form as the stone ascends. When the stone leaves the hand its energy is actual, at its maximum height it is potential. As a form of potential energy it is a kind of power existent in the stone, but unexerted until the stone begins to descend. According to the theory of conservation this unexerted power is not a reality which abides in the stone by virtue of its constitution, but a power that may be lost, and lost as quickly as it was gained. When the stone reaches the ground it possesses no energy beyond a portion of the heat generated by the collision. The advocates of the conservation hypothesis tell us that the potential energy of the stone at its maximum height is a power to do work. We inquire what work, and are gravely assured that the stone has power to fall, which it could not do when it rested upon the ground! Let us suppose that when the stone leaves my hand I was standing on a covered coal-pit two thousand feet deep, and that I remove the cover as the stone descends, if, as Mr. Brooke affirms, the Conservation of Energy is a fact, it follows that when the stone has fallen through twenty feet it will remain suspended over the pit. By its ascent it acquired power to fall only twenty feet, not two thousand and twenty feet. The metaphysicians, so much belaboured by Prof. Tait and other physicists, have ventured to think that the force of gravity has something to do with the fall of the stone. I have certainly found myself unable, even with the aid of the scientific imagination, to form an intelligible idea of the reality supposed to be symbolised by the term "potential energy." The theory of the Conservation of Energy as now maintained by physicists is opposed in several respects to the doctrine of the conservation of force as held by Faraday. Stewart, Brooke, and others teach most explicitly that energy is not only constantly changing its form, but always shifting about from one portion of matter to another. If I mistake not, Faraday asserts the very opposite respecting force. He seems to teach that each material particle, into whatever combinations it may enter, retains all its original forces. "A particle of oxygen," he says, "is ever a particle of oxygen."

Mr. Brooke makes other admissions which are as inconsistent with the truth of the doctrine of the conservation as the one I have examined. These, however, I must leave for the present. I feel that the most satisfactory reply to Mr. Brooke's strictures would be to quote here, with two trifling exceptions, the portion of my article which relates to the conservation of energy. Those exceptions I will now name. First, I withdraw what I have said respecting Mr. Brooke's view of the nature of latent heat. My sole reason for not in this connection quoting more was that I had assumed his perfect agreement with Prof. Tait. In this it seems I was wrong, since Mr. Brooke declares that he is unable to derive any definite idea from Prof. Tait's statement. I am sorry that Mr. Brooke should have supposed that the omission of the sentence named was due to a lack of literary honesty. I wonder that it did not occur to him that another and more charitable explanation was possible. Secondly, I was in error as to the weights employed by Dr. Joule in one of his experiments for determining the mechanical equivalent of heat. But this error relates merely to the form, not to the ultimate result of the experiment; and consequently in no way invalidates my reasoning. Holding, as I do, that forces are both conserved and correlated, I feel no difficulty whatever in accepting the facts established by Dr. Joule. He avoids speculation regarding the nature of force in itself, and deals exclusively with its manifestations. Thus, his discovery of "the mechanical equivalent of heat" is the discovery of a relation between two classes of effects.

There is one misrepresentation in Mr. Brooke's review of my article I must here point out. He says, "the reviewer thus quaintly expresses the relations of force, energy, and motion:—A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." But this is not my doctrine. I am here dealing with the consequences of one of Mr. Justice Grove's assumptions, viz., that if we attempt to analyse our conception of force, viewed as the cause of any perceived motion, we can get nothing beyond some antecedent motion. Mr. Brooke complains that the misapplication of the term "force" has led to great confusion in physics. His own statements are nevertheless unsatisfactory, if not contradictory. He accepts the definition of force given by Faraday. But this so-called definition by Faraday is not definition at all. It merely tells us what force does, not what force is. Mr. Brooke adds that the definition "may perhaps with advantage be thus amplified:—Force is a mutual action between the atoms or molecules of matter."

But these molecular actions or motions are the effects of force, but not force itself. In no instance whatever can force be resolved into molecular motion. Mr. Brooke says, "One finds oneself occasionally brought by circumstances into an unwelcome generalisation. Thus the reviewer, speaking of the supporters of 'conservation' in the lump, says 'they take it for granted that force is motion and nothing but motion.' This the writer entirely and absolutely denies." Will Mr. Brooke show that this denial is in harmony with his assertion that "force is a mutual action between the atoms or molecules of matter?" I cannot. My reasons for rejecting the assumptions on which the doctrine of the conservation of energy rests are not noticed by Mr. Brooke. These assumptions I have shown belong to false and exploded metaphysics." A false philosophy of causation, it is easy to prove, has greatly retarded the progress of science.

I perceive that Mr. Brooke has used for reference one of a small number of copies of my article printed for private circulation. Unfortunately the paging does not correspond with that of the review. Had I only anticipated the pleasure of an encounter with Mr. Brooke, I would gladly have sent him the review itself. As Mr. Brooke is aware of what passed at a very recent meeting of the Victoria Institute, I cannot longer withhold my name.

Sale, near Manchester, June 26 JOHN MOORE

Water Analysis

IN NATURE for June 27, 1872, Mr. Wanklyn directs attention to the facts that his paper on water analysis appeared in 1867, and that in 1868 he gave some absolute errors obtained with his process.

Mr. Wanklyn then proceeds to say:—"We have never said that distillation of albumin with alkaline permanganate converted the whole of the nitrogen of the albumin into ammonia. The assertion in your article is therefore untrue." Mr. Wanklyn's ideas of truth are probably peculiar, for if he will refer to his paper of June 20, 1867 (Chem. Soc. Jour. vol. v. N.S. p. 448), he will find the following:—"Direct experiments in which a known quantity of urea, gelatin, and albumin were taken, have shown that all the nitrogen in these substances is obtainable in the form of ammonia when they are subjected to the treatment about to be described, and has disclosed the very singular fact that boiling with a caustic alkali liberates one-third of the nitrogen, both of albumin and of gelatin, in the form of ammonia, and that a subsequent boiling with permanganate of potash liberates the other two-thirds."

Not a word is said in the paper about carrying on the permanganate treatment to dryness, and the only reference to such treatment is on page 450, where it is stated that boiling to dryness with potash alone causes the evolution of a "full third" of the nitrogen as ammonia.

THE WRITER OF THE ARTICLE

Scintillation

CAN any of your scientific correspondents tell me whether the following observation has been published, and, if so, where?

By very slight squinting, or (as suggested to me by a friend) by a slight pressure on one eye, we obtain two images of a star as viewed simultaneously from two stations a few inches apart. We made the experiment some nights ago, and could detect no relation whatever between the scintillations of the two. This seems to explain how little trace of the phenomenon remains when a telescope is used, for in that case we have a sort of integration performed over the whole aperture of the object-glass.

G. H.

To Entomologists

HOW often is it that the entomologist has to regret the want of his net? The rare butterfly, by some curious perversity, is

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certain to be seen when the net is not at hand. How, under such circumstances, is the butterfly to be caught? I reply, wait till it settles, and then pick it up. Perhaps most of your readers will reply "Absurd, no butterfly will sit to be caught in that way." Try and see. Whenever the butterfly settles you may walk quickly up to within a short distance from it; the distance will depend on the nature of the butterfly. Arrived at a short distance from the butterfly, the motion must now be slow and even, and as the hand is slowly and steadily advanced towards the butterfly, it will take little or no notice of it, and may be easily picked up with the fingers. In Italy this spring I picked up in this manner both varieties of swallow-tails, as many as five when walking one afternoon. I have also caught in this way, whites, red admirals, painted ladies, peacocks, and many other smaller varieties. This manner of catching butterflies does look suspiciously like the old story of catching birds by putting salt on their tails. Before anyone condemns it, I ask him to give it a fair trial, and I have no doubt he will be astonished at his success. The only thing to guard against is any jerkiness in your motions. All your motions when near the butterfly must be slow and regular. Perhaps the butterfly may not sit long enough for you to approach it by such slow motions. If the butterfly does rise take care not to change your slow and steady motions, and it will take no notice of you, and will often settle again within a few inches of your hand. Often the butterfly takes no notice of you; at other times it seems sensible of danger, but generally contents itself by folding its wings as close as possible, as if to escape notice. When the wings are in this position the butterfly is caught with least damage to its plumage. But in some cases the wings are open. When that is the case I have sometimes folded the wings before picking them up, in order to save the plumage, so tame are they under this treatment. At first I thought it was necessary to approach the butterfly from behind, and keep out of sight as much as possible, but this I find, though an advantage, is not essential. Of course I do not advocate this plan of catching butterflies when a net can be got, as it is much slower and not so certain. Yet it has its advantages. You get the butterfly without breaking its wings or ruffling a feather, and if not a good specimen you can let it away unhurt.

J. A.

ERNEST T. CHAPMAN

MR. E. T. CHAPMAN has met his death by an explosion in his laboratory at Rübeland in the Hartz. A letter containing this sad news has the following particulars:—

"As you are probably aware, Mr. Chapman's work was always in the laboratory, and it was there on the 25th inst. (June) that the accident occurred. On the morning of that day Mr. Chapman had a conversation with the gentlemen here, and shortly after this, at 11 A.M., the bomb-proof building in which he was with three workmen exploded with a tremendous crash. Mr. Chapman may perhaps have informed you that latterly he has been chiefly engaged with the production of nitric methylæther, with which he has been making various experiments, and we cannot explain the catastrophe otherwise than that he was not thoroughly aware of the great explosibility of this substance. The workmen present having also all perished, it is unfortunately not possible to obtain any details. The force of the explosion was so tremendous that all the surrounding buildings have been more or less injured, and about ten people seriously wounded."

The substance I presume was nitrate of methyl. If so, this lamentable accident furnishes another proof of the treacherous nature of explosives which, like nitro-glycerine and gun-cotton, contain hydrogen and carbon associated with nitrogen oxides.

Mr. Chapman was only in his twenty-seventh year when his career was thus prematurely closed. A pupil of Hoffmann and Kolbe, he was a prolific author of original researches in organic chemistry. Perhaps the best known of Mr. Chapman's researches is his study of limited oxidation. This process, in his hands and those of others, furnished chemists with a valuable method of chemical diagnosis. The little work on the Analysis of Potable Waters, by Mr. Chapman and Mr. Wanklyn, is a well-known work of reference on this important subject.

Mr. Chapman was an enthusiastic worker. His manipulative skill was of a high order, and his acquaintance with organic chemistry very extensive, his researches in this branch of science being very numerous. If he had lived, and had an opportunity of continuous scientific work, it is impossible to doubt that he would have contributed his quota towards rescuing our country from the too just reproach of rapidly becoming more and more sterile in chemical discoveries.

His intimate friends esteemed him highly, for he was a man of varied culture and singular conversational power. It was always a matter of regret to all true friends of science that a man of such proved ability and promise should have been compelled in a manner to banish himself in order to gain a livelihood. His letters show that even in the remote place of his exile his brain was busy with chemical and physical questions. He must have been killed instantly, and therefore without pain. And certainly as a brave and loyal soldier of science slain on the battle-field of the laboratory, his death, like his life, showed his unwearied devotion to science. We can ill afford to lose such men.

FREDERICK GUTHRIE

DR. WILLIAM STIMPSON*

DIED, at Ilchester, Maryland, May 26, of consumption, Dr. William Stimpson, Secretary of the Chicago Academy of Sciences, in the forty-second year of his age.

The announcement of the death of Dr. Stimpson will be received with profound regret, not only by a wide circle of friends here, but throughout the country. Science has lost an assiduous cultivator, the value of whose labours was recognised in both hemispheres.

For the second time in the course of a few years the Chicago Academy is called upon to mourn the loss of an accomplished secretary. Under Dr. Stimpson's energetic supervision, the collections gathered within its walls at the time of the great fire, in magnitude and importance ranked fifth in the United States, and so admirable were his arrangements that they were flowing in from every quarter of the world. Their total destruction on the morning of the 9th of October last was a terrible blow to the secretary. His private losses, too, were beyond computation, embracing as they did a choice scientific library, gathered at different intervals—many of the volumes being out of print, and many of them being presentation copies from the authors—and also all his manuscripts, including those of the Government Japan Expedition, of which he was the naturalist, which were written out for publication, and were copiously illustrated by drawings, many of which were engraved. Thus, in an hour, perished the results of twenty years' unremitting scientific labour.

To show the high estimation in which Dr. Stimpson was held by men engaged in kindred pursuits, it may be stated that, at the instigation of Agassiz, the results of the deep-sea dredgings of the United States Coast Survey were passed over to him for description—a task calling for the most exact and extensive knowledge.

After the terrible calamity to which we have referred, Dr. Stimpson remarked that he had not the heart to attempt to enter upon his life-work again, but would devote all his energies to the restoration of the Academy.

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* From the Chicago *Inter-Ocean*, Communicated by Mr. J. Gwyn Jeffreys.

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